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The Significance of Progress in the Social Sciences to Agriculture

By

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I shall not attempt in this brief summary to discuss progress in the whole broad field of social science. It seems to me more useful here to think of developments in the Department of Agriculture and the State colleges and universities than to wander too far afield. Joseph Schumpeter's recent book on the history of economic analysis alone runs to 1200 pages not to mention the discussions and controversies growing out of it. Jacob Viner refers to it as "an over-ambitious book" and comments that there runs through it "a vein of pretentiousness and of intellectual arrogance."

I must disclaim at once any feeling of intellectual arrogance. One cannot, as I have done in the past few years, examine the developments in the Department of Agriculture without being impressed by the great progress that has been made; in the improvement of basic data; in the development of analytical techniques and in the application of research findings to practical problems. My comment here refers principally to economic research. Progress in the natural science fields, I am sure, has been similarly impressive, but that is not the subject of our discussion here today. You have, of course, been hearing about it from people eminently well qualified to discuss it.

I do want to comment later on the problems and possibilities of more effective collaboration between the natural scientists and the social scientists since that, in my opinion, is one of the directions in which further progress can and should be made. In the main, the work carried on in the agricultural research institutions implies practical applications though there is, of course, a large and growing field for basic research. To the extent that the emphasis is on applied research, there is obvious need for integrating the contributions that can be made by both the natural scientists and the social scientists.

Before attempting to describe the developments in the social sciences, I would like to point out some of the differences in the problems faced by researchers in the social sciences and those that are characteristic of the natural science fields. However, it is easy to exaggerate these differences. Some years ago, we had at the University of California a continuing informal conference on the problems relating to conservation research and policy. Among the participants were soil scientists, foresters, chemists, historians, economists and various others. Most of us, I think, were more impressed with the similarities and interrelations of research interest than by the differences. However, there are differences that are important.

The social sciences are concerned primarily with people and their behavior in producing, selling and using economic goods and in their relations with each other. This introduces two factors that affect research techniques markedly. First, there are few types of social science research in which the effects of

^{1/} Lecture given at the Graduate School of the U. S. Department of Agriculture, Washington, on February 2, 1955.

other factors than the one under study can, to use a hackneyed expression, be held constant. The effort to measure and allow for influences exerted by factors other than the one of primary interest must, in most cases, be implemented by the use of statistical techniques rather than by controlled experiments. Secondly, since the social sciences deal with the behavior of people, the decision-making powers of the people themselves must be taken into account.

If a chemist combines two elements or compounds, he can predict with great certainty what the result will be; and it will be the same if he repeats the process a year or ten years later. A physicist can measure the speed of light and the light waves will not, on their own account, decide to move at some slower or faster rate or in some different direction. When the behavior of people is the subject of study, no such precision of result can be assumed, even though the surrounding conditions seem to be similar. Usually, even the factors conditioning the reaction will not be the same.

Few of the social sciences can be sharply segregated. People do not necessarily react logically. A customary economic relationship may be greatly distorted by psychological factors, changes in the level of economic literacy, government policies, or even by changes in technology which have resulted from progress in the natural sciences. For example, the increase in spendable income that occurs in wartime does not exert its normal influence on prices. People hold more of it in savings, perhaps with the result that its inflationary effect appears at a later time. Spending may be held down by price controls, rationing and patriotic appeals, or consumption may be stimulated by subsidies advertising and propaganda.

Habit and style changes may affect consumer attitudes and behavior significantly. For example, people have shifted away from wheat products and potatoes and toward meats, fruits and vegetables, not only for health reasons but because the slimmer silhouette has become popular. In the matter of economic literacy, it used to be fairly safe to predict a large increase in potato acreage after a year of high prices. The ups and downs in potato production and prices were striking and notorious and, to some extent, predictable. But farmers have become more aware of these tendencies and are now less likely to behave in the same way as they did in earlier periods when they were less aware of the factors affecting the prices of their products. Coupled with these decision-type changes are the well-known uncertainties resulting from weather, disease infestation, and so on.

These peculiarities of social science research point up sharply one important difference that is often overlooked, though it affects profoundly the atmosphere in which the two types of investigation are carried out. In many types of natural science research, the investigator is fully warranted in spending years, or perhaps even a lifetime, in refining his results, increasing the precision of measurement, and so on. If he gets the right answer, he makes a contribution to the body of knowledge in his field that will stand perhaps for all time or at least for a very long period.

Seldom can the social scientist expect, except perhaps in the field of history that his results will have such lasting value. Consequently, the degree of precision he is warranted in striving for, already severely limited by the nature of his research materials, is further curtailed by the fact that the specific conditions to which his research relates are not likely to be reproduced or reproducible in later periods.

Only in very fundamental types of relationship involving large aggregations is he warranted in spending great amounts of time and effort in developing extremely high precision in the measurements he works with. He is likely to be more concerned with fairly close approximations that can be derived quickly and used promptly. Thus, there is a significant difference in the time element as between the social sciences and the natural sciences.

Though these differences are readily apparent, they can easily be exaggerated. Precise measurement is far from easy in the natural sciences, and in many of them very similar problems requiring highly refined statistical techniques are necessary. The science of statistics in fact has its roots in the biological sciences rather than in mathematics or the social sciences. Even in the natural sciences, control over some of the factors that affect results is often difficult or virtually impossible.

We need only to think of the problems faced in the effort to classify soils in terms of productivity, the measurement of results from the use of soil conserving practices, or the variations in assimilation of nutrients by different animals, to be aware of the difficulties of measurement that are inherent in natural science research as well as in the social sciences. Even in such a field as medicine, where the researcher may have the active and intelligent cooperation of the subject, and no problem of changes in motivation and attitude, the determination of such a relationship as that between cigarette smoking and lung cancer is presenting difficult and as yet unsolved statistical problems.

As much as a century ago, Thomas Henry Huxley outlined what he regarded as the four basic elements of scientific method, namely, (1) observation of facts; (2) comparison and classification of facts leading to induction to general propositions; (3) deduction from general propositions to facts again, so as to foretell facts in advance of observations; and (4) verifications of deductions by fresh observations. Huxley was seeking then to justify including the study of biological sciences in the educational program. However, his propositions still are significant in both the natural sciences and the social sciences. The pace is faster in the social sciences and the value rating of precision as against timeliness is lower. Yet both must be based fundamentally upon the observation of facts, on statistical generalizations and, if they are to deserve respect, on verification through fresh observations.

I have emphasized thus briefly some of the differences and similarities in the social and natural science approaches, mainly because of my strong conviction that each has much to gain through closer contact with the other. Further than that, I am convinced that the time has come when we must increasingly seek to devise workable ways of using the resources of a number of disciplines in studying the complex problems we are faced with. That is not easy to accomplish as you well know. Individual research is much easier to organize and carry out than group research. In many fields, it has and should have an important continuing place. But group research is gaining in importance. The knowledge and skills required and attainable in respect to the many facets of such problems as conservation, farm policy and production adjustment cannot be encompassed by single individuals in the life span granted to us, even though that life span is longer than it used to be.

The alternative is teamwork on the part of specialists. Some but not all of them. I would be the last to disparage the importance of intensive, individual and highly specialized research on narrowly defined research projects.

Nevertheless, there are other fronts on which advances must be made as well. We have made considerable progress in the collaborative type of research, but I for one do not think we have yet developed satisfactory methods of organizing it and suitable techniques for carrying it out. Effective collaboration among researchers in a given discipline, though not easy or simple, is less difficult than the conduct of studies requiring skills drawn from a variety of disciplines. Yet the latter type of study is coming to be more and more necessary.

Let me illustrate briefly, first in the social science realm alone. Many studies, pronouncements and special pleadings are being put out in the field of farm policy. Mostly, these are coming from the economists. Yet, even the most superficial glance at these problems reveals the fact that the techniques of the political scientist are needed in illuminating various aspects of some of them, especially in evaluating the trends in political and social structure. The more subtle and probably more fundamental elements of these problems, those relating to value judgments, carry us over into the realm of philosophy. Often these are brushed aside all too easily and superficially. We speak of more freedom for the individual, nonmonetary factors in real income, stability versus "progress" and the changing mores of farm people, but how far have we dug into these concepts to see what they really are.

Some of the reactions of farmers, and, of course, of other groups as well, stem from psychological influences and may be quite illogical from an economic standpoint, yet very real in their effects on the success or failure, or even the adoption or rejection, of given farm programs. Full understanding of reactions of that kind is not likely to be achieved through the researches of economists alone. Here the psychologists and political scientists have a role to play though probably not so much within the governmental agencies as in privately supported research organizations. Unfortunately, studies carried out by such "other-discipline" researchers all too often are so lacking in contact with other types of reality that they do not serve as adequately as they might to strengthen and deepen the understanding derived from the more familiar and directly applicable studies carried on by economists and political strategists.

There is a place likewise for the anthropologist and the sociologist. In recent years, there has been a good deal of interest and writing about the very heterogeneous group known as "low-income" farmers, but we do not really know very much about the aspirations, abilities, handicaps and satisfactions of that group. We assume all too easily that they want the different kind of life we think they logically should want and that they could and would readily make changes in status or location if they were given opportunity. We may demonstrate fairly easily that a shift, perhaps to an urban occupation, would increase economic returns to them and improve the allocation of national resources, but also we may ignore some intangible values that rank high with them. In this realm there is a fertile and relatively unworked field for types of study that are now almost untouched in the Department of Agriculture and not much developed in the colleges and universities.

Of similar importance are the barely scratched problems resulting from the rapidly changing structure of farm and community life. Commercial farming, social relationships and community institutions are being greatly modified. The implications and probable later results of these changes are not well understood, nor is there as yet an adequate body of information for constructive planning in this realm. Also, we have a large, unstabilized and poorly housed farm labor group. Its existence is an economic and historical accident. Few commend it as a satisfactory way of performing necessary functions. But mostly it is not a problem

the individual employer can solve, no matter how good his intentions. It is far easier to damn it than to undertake the laborious task of devising methods and types of adjustment that will help in improving a situation that is generally regarded as deplorable.

These are only a few of the interconnections among the social sciences themselves which call for a higher degree of integration and collaboration in research. Similar joint interests appear as between the social scientists and the natural scientists. They are so obvious that I hardly need mention them. Only a few brief illustrations will suffice.

The animal nutritionist is concerned with the physical effects of given kinds and amounts of nutrients. The economist thinks in terms of the costs of different combinations and of their relation to the price of the product. The physical effects of a given combination of feeds do not change importantly, but the relative prices of feeds do change. The intensity and kind of feeding that will yield optimum net returns varies not only as a result of shifts in the relative prices of cost factors but also from changes in the relation between costs and the price of the end product. With butter or fluid milk at a given price, the optimum rate of feeding of concentrates may be quite different than if butter and milk are 10 or 20 per cent lower or higher.

Much time and expense are warranted in determining the basic physical relationships. But beyond this, economic studies to determine how to use such findings in making entrepreneurial decisions are also needed. These economic studies may require that sliding scale models of one kind or another be worked out to serve as guides for decisions that may have to be made at frequent intervals. The working out of such basic analytical frameworks on the economic side may require as high a level of precision and scholarly competence as the underlying scientific determination of physical relationships, but its effective use will be in the form of rough approximations in which timeliness will outweigh precision. The natural scientist need not be so much concerned with the time element.

Similar problems arise in the application of fertilizers. Certainly, precise results are far from easy to arrive at even on the physical side of the problem. But, even granting full success in this realm, the problem of economic optima in fertilizer application remains to be solved. Some years ago a sub-committee of a group I was working with undertook to bring out a brief report on fertilizer policy. It was prepared, for the most part, by a very distinguished group of soil scientists. Undoubtedly, from a strictly soil science standpoint, it was admirable. But from the standpoint of economics, the errors and gaps in it were so serious as to be a matter of unanimous and grave concern to the economist members of the parent committee who were later faced with the problem of passing on its suitability for publication.

I need not labor the point further. The interconnections among all of the research fields relating to agriculture are so numerous and so obvious that few are unaware of them. They are evident in the problems of forestry, pest and disease control, agricultural engineering, irrigation and plant and animal science. In some fields, such as forestry, the current trend is toward the development of economic analyses within the area itself. Conceivably, other branches of the Department could move in the same direction. But that would tend to dilute the basic scientific work of the Department in ways that might be to its disadvantage.

Furthermore, there still would be large segments of the economic work that would not find an appropriate home in any other unit but would nevertheless have to be carried on. The skills of economic analysts are readily transferable from one field to another and are particularly well suited to the analysis of joint product relationships, which cannot well be dealt with through approach in terms of a single type of product. Even in forestry, where the case for specialized economic research is undoubtedly strongest, the problem of competing uses for land is of great importance and requires other approaches as well as that of forestry itself.

I have postponed, possibly too long, a return to the central topic assigned for this meeting. My only apology is that it seemed to me difficult, if not impossible, to describe even in a roughly meaningful way what we might call progress in the social science phase of agricultural research without discussing briefly its nature and its place in the over-all program. Even so, this brief summary will have to be almost wholly in qualitative terms. There are many government activities to which we cannot assign quantitative values or results. The best we can do is to describe them. Each individual will have to place his own estimate on the need for them and the value of the product. Costs can be roughly measured. Returns often are intangible and certainly unmeasurable.

It is hardly necessary to say that the major research and service contribution in the social sciences has been in economics, both in the Department of Agriculture and in the State institutions. That is natural, since economic problems bulk largest in the thinking of farmers and legislators, whether at the State or national level. Hence, they are the ones for which funds are most likely to be provided.

The economic studies and service work of the Department seem to me to fall roughly into three major categories:

1. The collection of basic data. Here the Department has, in my opinion, made its greatest contribution. Without this enormous body of raw material for analytical work, most of the agricultural economics research both in and outside the Department would have been impossible. The United States now has a growing body of basic data that is incomparably better than that of any other nation in the world. It includes not only estimates of crop production and utilization but vast aggregations of data on prices, incomes, costs, mortgage debt, foreign trade, food consumption, farm employment and so on. Much of it is a joint product of the Department of Agriculture and the Bureau of the Census. Other parts, especially the price and income series are derived directly from the Department's own field surveys and other sources. The quality of these data still leaves much to be desired, but there has been steady and rapid progress in bringing them to higher and higher levels of adequacy and completeness, especially from about 1920 on.
2. The Department has made a sizable contribution in the form of analytical studies. This phase of its work is less fully developed than its data gathering activities and perhaps always will be. The analytical work has, in my opinion, suffered a serious setback in the decision to break up the Bureau of Agricultural Economics. However, there are obvious and perhaps inevitable limitations to the amount of strictly analytical

work on broad social problems that can be carried on by a government agency. That limitation is not confined to the Department of Agriculture. It exists also in the Department of Labor, in the Department of Commerce, and no doubt in other agencies as well.

3. There has been a very large development of what is sometimes called "operating research," that is, quick assembling of information needed in making administrative decisions, and rather rough and hurried analyses pertaining to some immediate problem. The trend in the last year or two, and perhaps in most of the years since 1930, has been in that direction. Those who are familiar with that type of work are well aware of three of its characteristics. First, it is necessary, in a government so heavily involved in the management of agricultural affairs as ours now is. Second, refinements and precision must often be sacrificed in the interest of timeliness and, third, it is very heavily dependent on the adequacy and scope of the reservoir of data, knowledge and analytical work built up before the need for it arose.

It is here that the losses resulting from breakup of the Bureau of Agricultural Economics are likely to show up most clearly as time goes on. That agency was, potentially at least, the reservoir from which specialized knowledge, qualified personnel, and the results of longer-term, more carefully made analyses could be drawn. The Bureau of Agricultural Economics was not, of course, the only source of such continuing and more basic research. Other branches of the Department were also contributing to it in a significant way.

The rough classification given above does not include a fourth phase of the Department's work which, though important, is less easily recognized. That is the gradual process of increasing farmer understanding of economic relationships. It can be doubted if this has been as much a conscious part of the Department's program during the past two decades as it was in the 1920s. Nevertheless, it has been a growing influence. No one who has worked with farmers over the past thirty or forty years will question seriously the gains in understanding that have been achieved, though this gain was not wholly, perhaps not even principally, a result of specific Department of Agriculture efforts. Regardless of the source of their knowledge, farmers do approach their economic problems now with far more understanding than they did only a few decades ago.

I should like now, in a rough way, to try to relate the developments in the social science fields to the general framework outlined above. It seems to me our purpose here will not be best served by attempting a detailed historical review, but rather by emphasizing changes in emphasis and over-all gains.

Some two or three years ago, H. C. and Anne Dewees Taylor brought out their large volume on the development of agricultural economics, the principal field with which we are here concerned.^{1/} It provides a good deal of detail about the

^{1/} H. C. and Anne Dewees Taylor, The Story of Agricultural Economics in the United States, 1840-1932, Iowa State College Press, Ames, Iowa, 1952.

about the evolution of agricultural economics as a field of study, though perhaps

not in as orderly a way as we might wish. So far as the Bureau of Agricultural Economics is concerned, I could not hope to add much to the excellent articles by Lloyd S. Tenney and John D. Black which were published in the Proceedings Number of the Journal of Farm Economics for November 1947. They are readily available to you. 2/

2/ "The Bureau of Agricultural Economics--the Early Years" and "The Bureau of Agricultural Economics--the Years in Between" Journal of Farm Economics, Proceedings Number, November 1947, pp. 1017-42.

In the broad field of scientific progress, the social sciences are recent, if we think of them as sciences rather than as systems of moral philosophy. No doubt some of you, as well as many others, have doubts that they have yet achieved a status that entitles them to be classed as sciences. Yet, their roots go back at least as far as the time of Aristotle and Plato, and the fumbling efforts to describe and understand economic relationships in the centuries between then and 1750 were perhaps not much more clouded by mysticism and metaphysics than those of the disciplines we now call natural sciences.

Nevertheless, hardheaded, careful efforts to observe, classify, measure, generalize and synthesize did develop earlier in the natural sciences, especially in the research program of the Department of Agriculture. Progress in these fields has been more solid and measurable but perhaps not more important in its effects on society. Until about 1900, there was not much attempt or opportunity to apply genuinely scientific procedures to the social science problems of agriculture. Up to that time, the funds provided were almost wholly in the natural science fields and it was there that professionally trained researchers first became available. However, some groundwork was being laid. Basic agricultural data began to be assembled as early as 1840 both in the Patent Office and in census operations. As early as the 1860s more carefully assembled data on crop production began to take shape and the recurring census enumerations, particularly from 1880 on, began to provide materials that were later to be of great importance to social scientists.

Social science problems and relationships were already becoming prominent in the thinking of farmers and their organizations in the late nineteenth century. But they were not looking to the Department of Agriculture, or to social scientists outside the Department, for help in solving them. The Department and the social scientists had little to offer, even if they had been consulted.

The years 1900 to 1920 were the formative period for the work in agricultural economics. But at that time, interest centered mainly on the economics of the farm unit, rather than on the larger national and group problems that came into prominence later. There was a reason for that. The concern over prices and the general economic organization of society receded into the background during the first decade of this century. Farm prices were rising rapidly. The farmer who could produce more efficiently on his own individual farm had a fair chance of success even if no momentous changes were made in the institutional setting or in national policies. Farmers were, to be sure, much interested in Teddy Roosevelt's trust-busting campaign and in food and drug laws, meat inspection, and so on, but on humanitarian and philosophical grounds, not because these measures were likely to increase farm incomes significantly.

This emphasis on study of the farm unit was approached in various ways. Hays, Warren, Boss and others were coming at it by way of Agronomy. They were familiar with the techniques then available in the agronomic sciences but not with those of economics. Consequently, they improvised as they went along, devising the cost-of-production route technique, the farm management survey method and some rudimentary approaches to the combination of enterprises. At about the same time, W. J. Spillman, whose background was in mathematics, began work on much the same type of problem.

H. C. Taylor and T. M. Carver were beginning to study and teach agricultural economics using as a background their training in economics, emphasizing the principle of diminishing returns, Ricardian rent theories, and some of the ideas of Francis Walker in respect to profits. Carver also gave attention to the history of agricultural development and Richard T. Ely, at the University of Wisconsin, was exploring and teaching about the institutional and economic aspects of land tenure. However, most of these early studies, whether undertaken by agronomists, mathematicians or economists, tended nevertheless to emphasize the problems of the individual farm rather than aggregative relationships. The data and techniques for such larger-scale studies were not yet available, even if there had been an active interest in undertaking them. Only toward the end of the second decade, when war demand and inflation were disrupting customary price relationships, was there much active interest in problems of that kind. G. F. Warren began to present and popularize his famous price charts in the years just prior to the great price decline of 1920.

There had, of course, been a growing interest in the marketing of farm products. It had its origin outside the Department of Agriculture, and the principal study in that field prior to 1900, the Report of the U. S. Industrial Commission (1898), was a result of congressional action and was not carried out by the Department of Agriculture. However, some start on marketing studies had been made in the Department as early as 1894 or thereabouts, chiefly in the study of foreign markets for farm products. Some criticism of the paucity of economic research was also beginning to appear in the reports of the Secretary of Agriculture.

Not much real progress in marketing research was made until Houston became Secretary in 1913. Then for the first time, the man in charge of the Department was a well-trained economist. But even then, and for some time thereafter, the marketing research undertaken was largely descriptive rather than analytical, except for that pertaining to the establishment of grades and standards, which was at least as much technical as economic. However, the crop and livestock reporting activities were put on a much more adequate basis from about 1915 on. Research and changes in organization looking to the improvement of crop estimates were important in the period prior to 1930 but came to be overshadowed by the operating activities of the Division of Crop and Livestock Estimates in the 1930s and after. Only recently has there been a revival of study and consultation looking to basic changes in the methods of collecting and interpreting such data.

During this second decade of the 1900s, there was other important progress in the field of agricultural economics, but mainly at the preparatory level rather than in actual research output. H. C. Taylor, Richard T. Ely, and B. H. Hibbard were training and influencing a group of graduate students at the University of Wisconsin who were later to exert much influence on the quality and scope of agricultural economics work in the Department. But the time was not yet ripe for them to enter upon the kind of program that was shortly to result in

the great development of data collection and analysis that was to mark the 1920s.

Interest was shifting from the rather narrowly conceived fields of farm management and marketing to the whole range of economic relationships affecting agriculture. Gray and Baker were turning to land economics, Stine to historical and price research, Holmes to a new type of farm management analysis, and Galpin was making some start on a descriptive type of rural sociology. John D. Black was getting ready for the extensive and diverse training program he was later to carry on at Minnesota and Harvard. G. F. Warren, too, was shifting away from his earlier preoccupation with farm management and becoming interested in price relationships.

With the severe break in farm prices that came in 1920 and after, the climate in which the new field of agricultural economics was growing became almost ideal for rapid and significant progress. H. C. Wallace, who was much interested in the economic problems of agriculture, became Secretary of the Department. H. C. Taylor, a primary leader in the field, was brought in to pull together and lead the various phases of economic research then underway, and to initiate new ones. The Congress was acutely aware of the price, income, credit, and marketing problems facing agriculture, and farmers and farm organizations were avidly seeking economic data and analytical material. Even the state legislatures were evincing an interest in the research side of the problem which had not characterized their attitudes in earlier years.

Taylor, as the first Chief of the Bureau of Agricultural Economics, proved a wise and able leader. He developed a strong team of enthusiastic and relatively well-trained division chiefs and others, many of them already exposed as students to his way of thinking. These were soon supplemented by others coming out of Black's group at Minnesota, Warren's at Cornell, and from various other places. In-service, graduate training was provided and some new fields were opened up. Galpin came in to head up a new unit dealing with the problems of rural sociology and rural life. New work was undertaken in the fields of credit, taxation, factors affecting prices and the organization and functioning of marketing organizations.

Even though Taylor himself was soon forced out as head of the Bureau of Agricultural Economics, the impetus given in the early 1920s, partly substantive and partly organizational, continued to carry the work forward at least until 1933. The drive for more and more activity in the study of economic relationships was, of course, intensified by the continuing unsatisfactory price, income and credit situation and the vigorous political struggle over the McNary-Haugen Plan. As the depression of the 1930s put agriculture in an even more desperate situation, economic studies were given still further emphasis, especially after the failure of the Farm Board gave rise to a search for alternative methods of attack on the farm problem.

What then can we say in a meaningful way about the nature and extent of the progress made between 1920 and 1933? While I regard that period as the one in which we have made our most important and fundamental progress from a technical and scientific standpoint, I cannot pinpoint it sharply. In rough, broad categories, I would indicate the gains somewhat as follows:

1. Agricultural economics had come to be recognized as a field in which first rate professional training had a place. Observation, generalization, testing of results and other scientific procedures were being developed though not in the same way nor to the same level of accuracy as in the natural sciences.

2. The collection and processing of basic data had been brought to a more adequate level and the initial impetus for continuing improvement in their scope and quality had been provided.
3. Research attitudes, training, and esprit de corps had been markedly improved.
4. Research horizons had been widened and many new kinds of problems were being brought under study.
5. Statistics, one of the basic scientific tools of the social sciences, was being more and more emphasized and techniques were being refined. More emphasis was also being put on adequate training in the principles of economics and their use in agricultural economics research.
6. A good deal of progress had been made in the identification and measurement of the factors affecting farm prices. In conjunction with this, the agricultural outlook program had been launched. Whatever its limitations from the standpoint of accuracy and reliability, that activity has long been recognized as one of the most effective devices for increasing farmer understanding of economic relationships.
7. New and more analytical approaches to the study of agricultural marketing had been initiated. For example, studies of the relationship between size of marketing unit and the cost of handling. A start had also been made in the study of land problems and policies. In the field of farm management, the budgeting approach had been developed and some start had been made in the study of type-of-farming areas.

Broadly speaking, the over-all achievement was that an important research institution had been built up, one that had already gained the respect of other statistical and research agencies in and out of the government. In the social sciences other than economics, not too much had been accomplished, except in the development of historical price series and similar semiservice lines, except for the important contribution of L. C. Gray in his History of Agriculture in the Southern United States to 1860. Sociological studies had not progressed very far and little, if any, work was apparent in such fields as political science, philosophy, and so on. Most of these, for reasons already mentioned, are not well suited for study in a government agency. Though their subject matter has a bearing on many agricultural problems, it gives rise to so much controversy and criticism that sizable appropriations for carrying on work of that kind are not likely to be made.

Development of Social Science Work from 1933 on

What may be termed a third period in the evolution of social science research and service in the Department began in 1933 and has lasted in one form or another until now. So far as we can now see, it seems likely to be continued for some time to come. The demand for economic data and analytical material increased enormously. The work already done in the Bureau of Agricultural Economics and related agencies proved a tremendous asset to the administrators of the new agencies then being set up. It can almost be said that, without that great reservoir of information, the programs undertaken in the 1930s could not have been launched or carried out.

But the new program was not an unmixed blessing for the Bureau of Agricultural Economics as a research agency. Progress was made, but mainly not in the Bureau itself, and mostly not in basic studies of lasting significance. The progress was mainly in recognizing new problems and opening up new fields of inquiry. Much of the research was on immediate and pressing problems and a good deal of it was of the operational, day-to-day type which might or might not be pushed to the stage of general distribution. The earlier work provided both a reservoir of information and a considerable number of trained and experienced people who knew how to gather and analyze data.

I do not make this comment in disparagement of the kind of shift that occurred at that time. There are times when the emphasis should be on the task of laying foundations, and there are other times when part of the crew may well be called away for fire-fighting or rescue operations. The need for such emergency types of activity was declining in the late 1930s, but a further change in emphasis made in 1938 created a new type of confusion so far as the basic work of the Bureau of Agricultural Economics was concerned. This was the assignment to it of a policy-planning function in the fall of 1938. This, as Black points out, ^{3/} was to some extent a resumption, in a more formal way, of a relation-

^{3/} The Bureau of Agricultural Economics—The Years in Between," op. cit., pp. 1033-37.

ship between the Bureau and the Secretary's office that had existed informally during the regime of H. C. Taylor.

I agree fully with Black's basic conclusion that ". . . the Department of Agriculture needs a strong general staff, but . . . the Bureau should not be that general staff." Certainly the kinds of data and analyses that can come out of a research organization such as the Bureau of Agricultural Economics was intended to be are of utmost importance in policy making, but if the research agency itself must formulate and recommend policy, the objectivity and quality of its research are bound to suffer. Furthermore, the assignment of such a function to a research agency inevitably makes it subject to political pressures that not only detract from the quality of its work but may even jeopardize its continuing existence. Studies in the social sciences, relating as they do to matters on which legislators and farmers have strong emotional attitudes, are at best more vulnerable than those in most natural science research fields. This argues for special efforts to insulate the basic research from the intense pressures that center about the Secretary's office in carrying out its policy-making functions.

The amount of time available here does not permit of going into the additional assignment made at that time, namely, that of providing leadership in the county land-use planning program. Here also, the program contemplated had merit, but it is questionable whether the Bureau of Agricultural Economics was the agency best suited for carrying it out.

I have been tracing, in the main, the developments in agricultural economics and the closely related field of political science. Though the latter field has not been specifically recognized in the research program, many of the studies and actions undertaken had close ties with political science and, by implication at least, reflected political philosophies that were popular in farm and congressional circles.

I am not prepared to say that formal studies in the field of political science can and should be undertaken in such an agency as the Bureau of Agricultural Economics. I do think, however, that there is need for clearer recognition of the political science aspects of many of the problems dealt with, and for a somewhat more conscious effort to stimulate such studies in nongovernmental agencies, as well as to use the contributions that can be made by them. There is perhaps a need even for some continuing internal study along those lines but in ways that will not bring them too much into the limelight. Agricultural administrators make many decisions that imply the acceptance of political science principles and criteria, but seldom with implicit recognition of what they are. This vagueness may and undoubtedly does at times lead to inconsistencies and inadequacies. Such applications of political science principles are much like those of the laymen in using economic principles, without recognizing that he is doing so or being aware of the contributions that more formal economic analyses could make to the solution of his problems. A department so heavily involved in the management of far-reaching public programs as those with which the Department of Agriculture is concerned needs the kinds of information and guidance that trained political scientists can provide.

I should like now to turn aside briefly from this more general summary to mention some modest attempts to venture into other social science fields, mostly in a temporary and experimental way. I have already referred to the effort to carry on some limited types of work in the field of rural sociology. That work, as carried along under Galpin and Carl C. Taylor has been on a modest scale and, in recent years, has centered rather heavily on the fields of population and farm labor. Some of the work carried on by O. E. Baker also was of sociological character though not formally tied in with that division.

In the late 1930s, H. L. Wilson undertook to stimulate interest in the cultural anthropology aspects of farm life and thereby to broaden and deepen the sociological approach to the problems of agriculture. This effort did not come to fruition in any important way. Wilson's leadership was lost to it when he became Director of Extension, and such impetus as remained was largely pinched off by the growing preoccupation with war.

Related to this was an educational rather than a research effort, the "schools of philosophy" program which Dr. Carl Teausch launched just prior to World War II. This was an attempt to encourage and stimulate thinking about the basic objectives of agricultural work and living. It was a rudimentary attempt to look beyond the purely materialistic features of the government farm programs and to consider values of other kinds than the ones measured in dollars and cents.

This program too was soon abandoned and had no important carry-over, though it touched on an almost unexplored field. The easy assumption that, if farmers could achieve and maintain parity of prices or income or some other material goal, all would be rosy, needs more than a little challenging as anyone who knew farmer attitudes back in the early 1900s is aware. Perhaps this is not an appropriate function for the Department of Agriculture, or even for the government, but nevertheless, these abortive ventures did constitute an attempt to draw on other social science disciplines. They have a place somewhere in agricultural research but possibly not in the federal agencies.

World War II brought a continuation of the emphasis on emergency and administrative types of economic analysis. That was logical and no doubt necessary. It did, of course, largely preclude a quick return to the pre-1933 types of research and service. A good deal of directly applicable analysis was carried

on in connection with the production goals program and the administration of price controls. Some work was done on forward planning for the postwar period, but the conditions generally assumed were so different from those that actually arose that these studies proved largely inapplicable and unrealistic. That, of course, is one of the major handicaps of such plans. Some advance planning is necessary but it probably belongs in the executive and policy-making units rather than in the research agencies. My own view is that the most important contribution the research agencies can make in this realm is to maintain a large and diverse reservoir of soundly conceived studies that can be drawn on quickly in shaping plans for meeting new situations as they arise. This includes, of course, keeping continuously available up-to-date and comprehensive collections of basic data.

The postwar years have seen a return to somewhat greater emphasis on fundamental research, particularly in the production field. However, the Department's social science research program still is heavily influenced by the demands on it which grew out of the depression decade and those of the war and postwar years. It had not yet settled down on a solid, long-term program of research activity when the reorganization of 1953 changed the setting for the conduct of research of that kind. Most of the older series of data were continued or expanded during the 1933 to 1953 period, and no doubt will be continued under the new organization. New series have been added and many of the old ones have been improved. New techniques in the handling of crop estimates are being explored.

A considerable body of research has also gone forward in other branches of the Department such as the Farm Credit Administration, Foreign Agricultural Relations, and so on. Organizational changes and war influences have changed the emphases and content of these activities from time to time. In general, they have seemed to fall more largely in the operating research category than in that of fundamental research, though there are undoubtedly some exceptions.

Let me turn very briefly to the recent reorganization. It would be presumptuous, I am sure, for me to venture dogmatic opinions about that. Those of us who are not here in direct contact with the new organizations have only fragmentary information to go on, and the plan is too new for it to show clearly either its advantages or disadvantages. Several brief discussions of it were published in a recent issue of the Journal of Farm Economics (in the February 1954 issue). 4/ With respect to the comments there presented, my own view

4/ "The Fragmentation of the BAE," by O. V. Wells, J. D. Black, et al and P. H. Appleby, H. C. Taylor, H. R. Tolley, R. J. Penn, and T. W. Schultz, Journal of Farm Economics, February 1954, pp. 1-21.

follows most closely the brief statement by H. C. Taylor. It seems to me that, while there is merit in organizing some types of research around particular problems, there is a great danger, as Taylor puts it, "of letting fundamental research 'fall between'" and of running out of basic background material. This danger has also been emphasized repeatedly by scientists concerned with what they regard as the overemphasis on "applied research" in the natural sciences. Taylor also emphasizes the possibility that there will not be enough qualified scientific people to make up the problem teams.

These defects, if they prove to be defects, will not show up prominently for several years. In the meantime, there seems to me a very real prospect that there will be a gradual deterioration in the quality and comprehensiveness of social science research in the Department, partly from the difficulty of recruiting genuinely research-minded people, if for no other reason. I hold no brief for the view that there is some innate superiority of the scientifically minded student of basic relationships over the man who deals with current problems. However, it seems to me to be common knowledge that the genuine researcher does not find himself greatly attracted by routine operating types of research. Hence I would expect some tendency for those interested in fundamental research to seek opportunities elsewhere and for the basic research of the Department to suffer as a result.

There are, of course, gains as well as losses. I have emphasized earlier the need for collaboration by social scientists and natural scientists. This may be more easily facilitated under the new organization than the old, but I am doubtful. Such collaboration is largely a matter of zeal and initiative on the part of the researchers themselves. It is not, as a rule, brought about by formal organizational arrangements. Hence it depends heavily on the atmosphere existing within each group.

The maintenance of a scholarly, research atmosphere seems to me more likely under the old bureau setup than under the present more impersonal one. Research organizations can easily become so large that the vision and enthusiasm of even a great leader cannot infuse the group with a spirit of teamwork and creative effort. I think there has been a real loss in the abandonment of the old bureau names. Perhaps one can become loyal and attached to a "Service" or a "Branch," but that seems to me to put an unnecessarily severe strain on his egoistic impulses. Furthermore, the new terminology is confusing and difficult for people who deal with the Department from the outside.

The old bureaus, such as the BAE, had personalities of their own. Membership in them meant something in the way of personal pride and group loyalty. I fear that some of that spirit has been lost and I doubt that a "Service" or a "Branch" can reinstall it. The Department has moved some distance in the direction of the single-purpose, impersonal type of organization that is characteristic of the research units of large corporations. However, even in the big corporations, there is a growing recognition of the need for greatly increased emphasis on basic research which is not directly related to immediate problems. That is not to say, of course, that there is no need for an overhead, coordinating agency in the research program of the Department. Obviously there is, but such agencies should be in the background rather than in the public eye, and there should be recognition of the need for pride and satisfaction in the accomplishments of a distinctive and appropriately named unit to which one belongs. Scientists either in the natural or social fields are not noted for their lack of egotism.

The above comments are, of course, purely personal views. Perhaps the time will come when I shall feel warranted in changing them. I have not been close to the new developments, as you here in the Department have been. Much will depend on the vision and inspiration the various administrators can supply. Research is after all a matter of men and their motivations and abilities, not of organization.

I have said very little about social science research in the colleges of agriculture and elsewhere, and the time available does not permit of going into it. In many respects, it resembles that in the Department. It has gained status and recognition in nearly all of the great universities. Research staffs are not only much larger than they were thirty years ago but they are very much better trained.

A good deal of progress has been made in the development of research techniques, in professional standards and in the attitudes of farmers and the public in regard to the work. Similar gains have, of course, been achieved within the Department of Agriculture. Some of the same gaps are evident in the work of the State institutions as those which have been mentioned earlier in describing the work here in the Department.

There is, of course, much still to be done before this, one of the newest of the fields of inquiry, develops methods, standards and reliability that are fully on a par with those of the older disciplines. Some of the older members of the profession feel that there has perhaps been a tendency for some of the newer work to be overconcerned with minute detail, at the expense of the broader, institutional phases of the problem. This is not to deplore a necessary and desirable striving for greater accuracy and precision but rather a fear that excessive concern with the individual trees may lead to a failure to understand the forest as a whole.

We have probably tended to split rather widely as between the very loose and unscientific studies of farm policy and the almost microscopic detail with which some researchers deal. There is an in-between area that needs also to be kept under study. It is in this realm that most of the practical working decisions have to be made. Also, we need, I think, to reach out more vigorously into the realms of historical perspective, value judgments and political institutions, all of which have a bearing on our problems.

Nevertheless, when we look back to the status of the social science phases of agriculture as little as forty years ago, and when we survey the quality of the articles and books now coming out, as compared to those of two or three decades ago, I think we must conclude that genuine progress has been made and that the contribution of the social scientists has been impressive.



